RANDOLPH (N.A.)

A NOTE

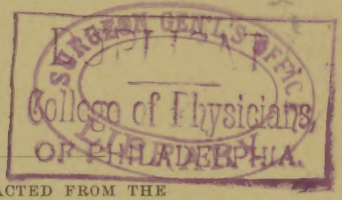
ON

THE IRRADIATION OF MOTOR IMPULSES.

By

N. A. RANDOLPH, M.D.,

PROFESSOR OF HYGIENE IN THE UNIVERSITY OF PENNSYLVANIA.



EXTRACTED FROM THE

TRANSACTIONS OF THE COLLEGE OF PHYSICIANS OF PHILADELPHIA,

MARCH 2, 1887.

PHILADELPHIA:

WM. J. DORNAN, PRINTER.

1887.

A NOTE

ON

THE IRRADIATION OF MOTOR IMPULSES.

By

N. A. RANDOLPH, M.D.,

PROFESSOR OF HYGIENE IN THE UNIVERSITY OF PENNSYLVANIA.

EXTRACTED FROM THE

TRANSACTIONS OF THE COLLEGE OF PHYSICIANS OF PHILADELPHIA,

MARCH 2, 1887.

PHILADELPHIA:

WM. J. DORNAN, PRINTER.

1887.

A NOTE

ON

THE IRRADIATION OF MOTOR IMPULSES.

By

N. A. RANDOLPH, M.D.,

PROFESSOR OF HYGIENE IN THE UNIVERSITY OF PENNSYLVANIA.

[Read March 2, 1887.]

About two years ago this question arose in my mind: If a man performs work with the muscles of (*e. g.*) his right hand exclusively, and to the point of fatigue, can he thereafter perform as much work, of the same nature, with the left hand as he could if the right had not been previously exercised?

It will be seen that this question relates in nowise to a comparison of the work of the two hands, but to an examination of the work which may be accomplished by one hand as conditioned by the previous exercise or non-exercise of its fellow of the opposite side.

It is evident that the answer to this query is dependent on intracranial processes solely, and that such answer would throw some light upon the functional independence or interdependence of the two halves of the brain.

In order to answer the question just stated, certain conditions are prerequisite. The subject of experiment

must have no conception of the object of the investigation, or he will unconsciously become a partisan of one or the other hand. He must, also, have a very strong inducement to exercise his volition to the utmost.

These conditions were fulfilled in the persons of some intelligent and vigorous convicts in the Eastern Penitentiary in this city. The stimulus was a money prize to the man who accomplished the most work in a given time. In the prosecution of these experiments I am under obligation to the courtesy and assistance of Dr. W. D. Robinson, physician to the penitentiary.

In the first series of experiments, rubber bulb syringes, identical in all their measurements, were used, and the amount of water which the men could transfer from one vessel to another in a given time was accurately measured and taken as proportionate to the work performed. Some forty observations were made by this method, with the uniform result that either hand could do more work when its exercise preceded than when it succeeded the similar exercise of its fellow of the opposite side. It was found, however, that the muscular effort could not be entirely restricted to one side of the body in this method, as great fatigue was always accompanied by a grimacing and writhing which implicated the muscles of both sides of the face and trunk.

A Morse telegraph was next used, the muscular movements in this case being restricted to an up-and-down motion of one finger of each hand, the number of such movements made in a given time being recorded on the usual long and narrow strip of paper as dots or dashes, in accordance with the celerity of contraction and relaxation of the flexors of the finger. The results of this series of experiments were uniformly confirma-

tory of those before obtained, but the method had to be abandoned as productive of great eye-strain in counting.

I then had made the instrument which is here shown. It consists simply in the clockwork and dial of an ordinary gas meter, to which a lever is adapted in such wise that each flexion of the finger is recorded by an appropriate motion of the index on the unit dial. The apparatus is fixed in a box, upon which the hand and forearm may conveniently rest. The lever projects through an aperture in the lid, and a glass plate in the side permits the records on the dial to be easily read off and noted. Six healthy prisoners, supplied with the incentives of cash and competition, were repeatedly examined by this means. To each, fifteen minutes were given to make the best record he could with (*e. g.*) the right forefinger, and thereafter the left forefinger was similarly exercised for the same time. On the following day the same process was repeated, always commencing, however, with the finger of the hand which had been used second on the previous occasion. No hand was ever thus employed twice on the same day.

The results were practically uniform. The man who for fifteen minutes flexed and relaxed his right forefinger with the greatest speed possible to him would, on the following day, accomplish, on an average, nearly ten per cent. less work with that finger when its exercise was consecutive to a similar exercise of the forefinger of the opposite side, than when its work was initial.

Usually more work could be accomplished by the simultaneous exercise of the two forefingers, than by their exercise one after the other. In such exercise of both hands at once, and apparently from some unconscious effort at rhythm on the part of the subject, it

was noted that the movements of the left forefinger were generally more active and rapid than when used alone, although in both instances the greatest effort at speed was evidently made. I am told that some pianists have a similar experience, inasmuch as they find it possible to exercise the fingers of the left hand more rapidly when the right is similarly engaged than when the right is inactive.

The relation of these facts to the observations of Dr. S. Weir Mitchell and Dr. Morris Lewis is largely supplemental in its nature. These observers, as is well known, showed that the knee-jerk is reinforced by any voluntary movement in any part of the body, and that this reinforcement was apparently due to such an irradiation of motor impulse from the active centres to other similar centres as placed them and their related muscles in a condition of heightened responsiveness to external stimuli. My studies not only tend to confirm these observations, but to show that the fatigue of one centre may induce a sympathetic fatigue in other centres.

These observations are of interest, inasmuch as they suggest—that the centres for volition, attention, and coördination (or one or more of these) are not, in their functional activity, bilaterally symmetrical and independent—that is, that these functions have not attained complete differentiation into right and left will, attention, or coördination; that, probably, the first effect of the voluntary activity of a portion of one cortical motor area is a stimulation of the corresponding portion of the other hemisphere—a stimulation that may result in its slightly premature fatigue; that apparently more work can be effected through the voluntary simultaneous exercise of two such portions of the motor apparatus than by their independent exercise one after the other.

[After the reading of the preceding paper:]

Dr. S. Weir Mitchell said: The remark that with fatigue comes an increasing tendency to convulsive or useless acts of facial or other muscles, is interesting. It seems to show that with feebleness comes increasing waste by overflow of motor energy on to distant ganglia. Perhaps in all states of weakness there is more or less of this tendency. Admitting the accuracy of Dr. Randolph's facts their explanation is difficult. It may be that the overflow of energy on to symmetrically related centres, or on to others, is competent to weaken them without being strong enough to cause motion; and whether this occurs as regards the opposite hemisphere, or only as regards opposite spinal centres, is hard to say.

(As an illustration, we may by a key close the current of two batteries, A and B, each competent to set in motion a mechanism. Resistances on the one circuit, A, so interfere as to lessen the flow of energy below what will move the mechanism. Meanwhile the other battery, B, runs to exhaustion. At last we call on battery A for a repetition of the full work done by B, and find A unable to effect the same work as that done by B, on account of having been partially disabled by its previous ineffective waste of energy.)

We are free to speculate as to the relative failure of one hand, the last in use, as due to there being but one centre originative of will signals to the lower ganglia, and itself capable of fatigue. If, however, we entertain any such view, it might be in a measure tested by exacting work from a non-symmetrical pair of limbs, as a foot and hand.

I incline toward the use of the overflow theory to explain the lowered capacity for work by one hand after exhaustion of the other. It would explain why in consentaneous use of two symmetrical parts more work is done than when they follow one the other. The overflow would be in this case valuable, and not damaging or wasteful. This leads me to relate an experiment which Dr. M. Lewis and myself left out of our paper on knee-jerk, but of which I am now sure enough to speak. When we use the maximum power of one hand on a dynamometer, the coinstantaneous use of the other hand

adds nothing to the result; and this form of experiment has been commonly used as a test of the reinforcing capacity of the opposite member. If, however, using two fingers, or the grip of the thigh adductors, on the bulb of a mercural dynamometer until great exhaustion occurs, and we then make a new effort at the moment of violent use of another member, the mercury leaps quite to the level attained during the first effort by unfatigued muscles. It does not seem easy to explain this fact, except by assuming that the overflow of energy usually wasted is in this case made efficient.

The question of muscular, and indeed of ganglionic, tone is brought forward in an interesting way by this experiment. When we strike the patellar tendon, a sudden, distant, voluntary act adds reinforcement. What is it that happens to the muscle or ganglion so influenced? Is it made more sensitive to impressions, or with this is there a slight flow upon it of motor energy? And if so, can we measure the effect, and thus influence what we conceive of as muscular tone?

For some time I have been engaged in discovering if these reinforcements do cause motion—*i. e.*, a slight preparatory muscular contraction making the subsequent volition, or other excitatory activity, more potent in its results.

I have been able, so far, to prove that in some spastic cases distant muscular effort, such as a grimace, really causes distinct and measurable movement in the extensors of the thigh and presumably elsewhere. This interesting discovery has been confirmed in New York and at Harvard. Whether in *normal* man remote motion is thus capable of causing slight shortening of all other muscles does not as yet seem clear. In my own experiments I obtained what seemed to be the same but slighter results than such as were seen in spastic paralysis, and hope very soon to solve doubts, and to be able to state my conclusions in more decisive shape. Upon what these may be, will depend much of our hope as to realizing clearly the true nature of muscular tone.

We often speak of nerve power as if there were a common stock from which are drawn the supplies needed by every active organ, and reason that it is unwise to try to carry on at once two functions which exact large expenditures—as digestion and intense thought,

or digestion and exercise. Practically the difficulty may be one chiefly of blood supply. This is illustrated in the not rare fact that some feeble people cannot digest except when at rest. These facts suggest the idea that perhaps Dr. Randolph's cases would lose ten per cent. of mechanical capacity after a period of exhausting mental labor or during digestion.

Dr. H. C. Wood said: The old theory which was used to explain the principle of counter-irritation, was that there is a certain amount of nerve force in the system, and that when by means of counter-irritation the nerve force is drawn to a distant point, it is removed from the inflamed part. Modern science does not recognize the truth of this theory, but it looks to me as though there is a certain amount of truth in it. Every one who has worked in a gymnasium will recall the fact that he cannot use the two hands simultaneously with the same force as he can when the two hands are used separately. A man who can put up a fifty pound dumb-bell with the right hand and a fifty pound dumb-bell with the left hand cannot at one time put up a fifty pound dumb-bell with each hand. This shows some relation between the nerve centres which we have not as yet gotten at. Dr. Randolph's contribution is an important one looking toward a final solution of this question, but I think that it has not gone far enough to enable us to form any theories with sufficient grounds on which they may rest to hope that the theories are correct.

I believe that when we use our muscles vigorously, two kinds of fatigue are produced. There is a local fatigue and a general fatigue. If a man uses the right arm vigorously, he not only fatigues the right arm, but also the whole body. I believe that if these investigations are continued, it will be found that after prolonged use of the leg, there will be loss of power in the arm, perhaps as great as after previous use of the other arm.

Of course, there is a temptation to speculate upon these facts, but the matter must be carried further before speculation will amount to anything. As suggested by Dr. Mitchell, the relation between muscular exertion and mental exercise should be studied. Each one knows by personal experience that when mentally fatigued he is

incapable of performing the usual amount of physical labor. This is probably independent of any question of overflow, and goes back to the higher cerebral centres and their relation.

Dr. Charles K. Mills said: It seemed to me while listening to the reading of this paper that certain well-known clinical facts in cases of brain disease have some relation to the subject under consideration. For instance, an old hemiplegic, if examined carefully, will be found to have not only the decided loss of power and accompanying conditions resulting from the lesion on the opposite side of the brain, but also a certain diminution of strength in the limbs of the other side; a condition which is not entirely due to the general loss of physical power present. The phenomena which are exhibited by certain spastic cases, or certain cases of unilateral spasm, also seem to me to have some relation to this subject. I have carefully studied the histories of certain cases of spasmodic infantile hemiplegia. In some of these cases, the autopsy has subsequently shown the existence of an irritative, destructive lesion in one hemisphere of the brain. If these cases are carefully studied, it will be found that in not a few the spasm has first appeared in one limb, or a portion of one limb, or in the muscles of one side of the body. After the lapse of months or years, the spasms increase and involve the whole of the original side, and after a time the other side.

We have other illustrations of the same idea in cases of dural spasm, of which there is at the present time an instance in Blockley Hospital, where the spasm was shown by operation to be due to irritation of the dura mater. In this case, and in another on which trephining was performed, the spasms were at different times unilateral and bilateral. Any number of illustrations of this kind could be adduced from clinical experience. They show that a lesion strictly local, involving only one hemisphere and only a limited portion of that hemisphere, will give rise to local spasms, and, after a time, to general spasms, the other hemisphere never becoming involved in any direct pathological process. Radiation of irritation from lower centres may, however, explain some of these cases.

Dr. H. Hartshorne said: I have given some thought to this interesting subject, looking at it from rather a different point of view from that presented so far to-night. The experiments of Dr. Randolph seem to confirm the conclusions of Drs. Mitchell and Lewis quite distinctly, but there is, I think, a good deal more than that in them. They show that there is not that differentiation of centres which has been asserted—that there is not an insulation, as we may say, of the centres. I should say there is coördination and unity, more than mere sympathy, especially of those of the two sides of the brain and of the spinal cord. With regard to the brain, we are all aware that our consciousness attests this unity, which is perceptional and volitional. Some simple evidence on this point is familiar to us. How is it that with the eyes shut we can place any finger of one hand against the corresponding finger of the other hand? This must be done solely under central guidance. With reference to Dr. Wood's illustration, it seems to me that the inability to lift two heavy weights at the same time is due not to the greater difficulty in the use of the muscles, but to the physical inconvenience owing to the structure of the body. It is awkward. The experiments of Dr. Randolph certainly point the other way. There is another curious experiment which I have frequently repeated. If you write on a blackboard with both hands at once, I believe that you can write your name more readily and certainly backward with the left hand than when the left hand is used by itself. You do not need to pay more attention to one hand than to the other. Here is an example of bilateral unity of volition.

The first result of Dr. Randolph, I should explain very much as is done by Dr. Wood, that is, that there is a certain amount only of available dynamic energy at any one time and place, and if that is drawn upon, the general sum of dynamic energy, and especially the energy in that particular part, is diminished. When one hand has done all that it can do the sum of the energy is lessened, and if there is a repetition of the work by the other hand the amount is less than it would have been if there had not been this consumption of power.

The second result is even more interesting, and, I think, involves psychical factors as well as those which are purely physiological. It

does not seem to me that intracranial processes include all in explaining these results. The energy belonging to the spinal centres has to do with forming the sum total of available energy at the time. In explaining the fact that the two hands working together perform more work without exhaustion than when they are used one after the other, there seem to be two other factors, attention and voluntary inhibition. We all know that fatigue is caused by attention. Dr. Randolph spoke of eye-strain, that is also brain-strain. It is said that those who in working the submarine cable translate the messages by watching the flash of light from a mirror, are only able to work one hour at a time. The physical effect of attention is shown by a curious experience which has been published in the journals—that is, that if a light be placed near enough to the eye to affect the pupil, yet allowing the subject to see beyond, it was repeatedly observed that if the attention was directed to the light the contraction of the pupil was decidedly greater than when the attention was directed to something beyond. There are also some curious results reported not long ago by Raggi. In experimenting with faint sounds, such as the ticking of a watch in a room where all was silent, he found that there were intervals when no sound was heard. These periods were as long as from seven to twenty-two seconds. The period during which there was audition was from seven to fifteen seconds.

Taking the view which I have already expressed in regard to the unity or coördination of the cerebral and spinal centres of the whole cerebro-spinal axis, it may be that the natural and spontaneous method of volition is for the impulse to descend and distribute itself symmetrically to the two sides, and that attention is necessary to prevent this and to make its direction oblique and concentrate it upon one side, and as a result of this effort of attention fatigue is induced.

I am not a believer in the normal functional inhibition of nerves, although this view is held by most authorities. It is apparent that we have three kinds of inhibition: one of the lower centres by the higher, the spinal axis by the brain; another form is pathological, of which there are many instances; and the third form is voluntary inhibition. I need say nothing more in the way of illustration than to refer to the great fatigue or strain which is induced by the

effort to restrain strong emotions. There are some circumstances in which it is the hardest thing possible to keep still. That is what I mean by voluntary inhibition. If it be true, as I have conjectured, that there is this normal symmetrical mode of volitional action from the brain downward through the spinal cord to the two sides rather than to one side, there is need to restrain the one not allowed to act. This effort to restrain one side is a draft upon the cerebro-spinal energy and this involves some fatigue, and thus there is less power if one act follows the other than if both acts are performed at the same time.

It seems to me that the greatest importance of such observations consists in their effect in modifying the idea which was formerly held as to the meaning of the term "nerve centre." Long ago, Flourens suggested that there is an equivalence of function for all parts of the brain. This view, of course, cannot be held. Later, Brown-Séquard formulated the idea that one hemisphere might do all that the whole brain could do, not in power and endurance, but in function. There has been published within a year a work by Luciani and Seppili on cerebral localization, in which they very clearly point out that a centre is not a point or a cell, or a collection of a few cells; but the idea might be illustrated by our use of the terms "centre of business," or "centre of fashionable residences;" while the business may be conducted largely in one locality, still it is not confined to that part. In one of the diagrams of the authors mentioned, the visual centre is shown by a dark place, the central portion of which is the darkest. This view explains many experiments where the destruction of what is called a centre does not prevent the performance of the function belonging to that centre. It seems to me to be an important advance in nerve-physiology to get rid of the idea of the insulation of centres.

Such experiments as those of Dr. Randolph are of great interest in connection with pure physiology, and also in connection with psycho-physiology, which has now almost displaced the old psychology in the schools, and which occupies a large part of the attention not only of physiologists, but also of psychologists.

Dr. Francis X. Dercum said: The thoughts that suggest themselves to me have, in a large part, been embodied in what Dr. Wood has said, and in the latter part of the remarks of Dr. Hartshorne. I think it extremely probable, and have so expressed myself in a paper read before the American Neurological Society, that the nerve centres are not centres defined with anatomical and mathematical precision. The centre is simply the point of greatest functional activity. Take, for instance, the centres for motion, and the sensory areas in the cortex; these are nothing more than the pointss where the various motions and sensations are focussed. These points are, to me, nothing more than the gateways of ingress and egress to the general cortex. It also seems to me that the nervous system being a whole, if any one part acts it must necessarily tire the whole. If one part acts, all the other parts must be fatigued; and the evidence is all the time increasing that the interdependence is so great and intimate that this can be held not simply as a mere speculation, but can be regarded as a fact. To such conclusions does the work of Dr. Randolph, and also that of Dr. Mitchell and Dr. Lewis, incline.